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On Books

Emergence of Relations and the Essence of Learning: A Review of Sidman's *Equivalence Relations and Behavior: A Research Story*

Duane M. Rumbaugh
Georgia State University

Sidman addresses two very important questions in *Equivalence Relations and Behavior: A Research Story*: What are the bases of behavioral competence? And how do units of learning become related? His book is not a traditional treatise on topics of conditioning, behavior modification, or how behavior comes to be controlled by specific stimuli. Rather, it is about how new relations emerge so as to provide the foundations for basic competence for "words and other symbols versus things and events" (p. 2). His challenge is to account for the transition whereby stimuli, which initially are arbitrary, come to be associated with referents and thereby acquire what is called *meaning*, in common parlance. Notwithstanding common usage, Sidman eschews all cognitive connotations for the word *meaning*, and defines it as a special instance of word-referent equivalence "culled from everyday experience" (p. 7).

Sidman likens his challenge to that of accounting for *induction*, which he holds to be a behavioral, not a logical, process (p.16). Through use of procedures whereby the subject is given one stimulus, A, associated with B, and B, in turn, is related by similar procedures to Stimulus C, Sidman then asks whether "equivalence relations" are present. If there are equivalence relations, A will relate to "C, C to A and to B, B to A, A to itself, B to itself, C to itself" (p. 16). In sum, the relations that emerge have *not* been the target of specific reinforcement in the subject's training history. They have been induced—or produced by Sidman's paradigm.

The book recounts the story of how an understanding of emergent relations and competencies was achieved through studies in his teaching-research program with mentally retarded subjects. Although children normally accrue vast networks of relations between stimuli and events, those with mental retardation typically do not. Consequently, by learning how to establish those networks, Sidman and his students contribute richly both to the cultivation of competencies by their subjects and, more generally, to an understanding of real-world human behavior.

Geschwind's (1965a, 1965b) neurological perspective emphasized the

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Sidman, M. (1994). *Equivalence relations and behavior: A research story*. Boston, MA: Authors Cooperative, 606 pp.

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Address correspondence to the author at the Language Research Center, Departments of Psychology and Biology, Georgia State University, Atlanta, Georgia 30303.

close linkage between cross-modal equivalences and language, and posited that cross-modal (i.e., auditory-visual) equivalences make language in its most basic form possible. In response to this hypothesis, Sidman designed a study to demonstrate whether or not learned auditory-visual equivalences were sufficient for reading comprehension to emerge without explicit instruction in reading comprehension. The subject, mentally retarded, could already match spoken words to pictures and could even name them at the beginning of the experiment, but he could not read. Would establishment of equivalence between spoken words and printed words lead him to reading comprehension, and, given his ability to name pictures, would auditory-visual word matching provide for the emergence of oral reading (p. 29)?

At this point the high excitement that can be associated with research is vividly recounted. When the subject *did* read, a wave of excitement washed over those in the laboratory. Specific sensory modalities were not germane to reading. (Even Geschwind allowed that deaf persons could learn to read.) What was critical was that the subject learn two sets of conditional discriminations in a manner that supported the emergence of new conditional discriminations (p. 35). And although Sidman, in support of the behavioral tradition, discounted the importance of *mediational* theory, it was in response to Geschwind's theory that new discoveries were made—those that provided the foundation for the rest of Sidman's book.

In my view, Sidman recounts all too casually the profound change in his perspective on learning. In his view, the matching-to-sample procedures he employed were basic to the establishment of equivalence relations "among *features* of the environment" (p. 37, italics added). Accordingly, Sidman concluded that, "*stimulus–response* relations therefore gave way in later publications to the more descriptively accurate *stimulus–stimulus* relations, and the relatively sterile notion of *connection* gave way to the productively defined *equivalence relation*" (p. 37).

Psychologists have only one option for answering questions, and that is by measuring behavior. In a functional analysis it is the source of behavioral control that is the essential issue. Notwithstanding, the assumption that behavior is controlled by relations between stimuli, responses, or both appears to signal a profound shift in perspective from the earlier dependence on the three-term contingency (stimulus–response–reinforcer).

Later in the book, however, Sidman concludes that equivalence relations emerge because of the natural consequences of reinforcement and that this "is consistent with a conception of equivalence as a primitive function," (p. 389). Instead of discounting the three- or even an *n*-term contingency, he builds a new perspective of what it can produce—and that is the formation of equivalence classes and relations between stimuli and stimuli, between responses and responses, and between stimuli and responses as well. He even allows that "Discriminative stimuli, for example, will function as reinforcers and reinforcers will function as discriminative stimuli" (p. 393). In so doing, he strives to account for the "first instances" of behavior, how adaptation afforded by learning can extend in novel ways to new contexts and environments, and how the specification of contingencies by rules comes about (p. 567), as posited by Skinner.

Sidman's view is, then, that reinforcement has far more comprehensive consequences than just increasing the probability that a response will occur upon the presentation of a discriminative stimulus. Instead, reinforcement enables the emergence of equivalence relations, from which the meanings of stimulus events can be established and from which inductive inferences are made possible.

And here a puzzling question must be addressed. Particularly in the last portions of his book, as Sidman pon-

ders the origins of stimulus relations, he even considers "that the equivalence relation, like the relations we call reinforcement and discrimination, is a product of survival contingencies" (p. 388), and "that we form equivalence relations because we are built that way" (p. 389). Sidman has trouble with concluding that equivalence relations are established by some acquired or newly learned response, which might be named "equivalencing." He questions how the equivalencing of exemplars that share only common relations could emerge "in the absence of a highly complex verbal repertoire," one not possessed by people with severe mental retardation, or by animals. Notwithstanding, they do form. How? Why?

Sidman rejects equivalencing, relational frames, and mediation as acceptable means of accounting for stimulus relations and asserts that there is sufficient explanation in the observation that "equivalence relations in behavior are instances of the class that elementary mathematical set theory describes" (p. 558). Sidman argues that the parallels between equivalence relations and elementary mathematical set theory (which includes the concepts of identity, symmetry, and transitivity) are sufficient to their explanation (p. 553). But he also recognizes that although "Elementary mathematical set theory describes the abstract properties of equivalence relations" and is of value in determining whether "any particular event pair belongs to an equivalence relation, it is silent about the origin of equivalence relations themselves" (p. 553).

Sidman also appropriately observes, however, that tests for equivalence relations do not constitute definitions of their requisites or an explanation of them. They only declare their existence. He rejects mediational theories as awkward and considers them to be "inelegant" efforts that rely upon very fragile props. For instance, he argues that mediational theory could not account for AB and BC symmetry without "accepting the reality of backward conditioning," which at best is an uncommon phenomenon or mechanism (p. 380).

Sidman, in the final analysis, attributes the origins of equivalence to the natural processes of reinforcement (p. 553). But one might well ask, reinforcement of what? There is no "equivalence" response in the traditional sense to be reinforced during training. Although equivalence relations do emerge, they are neither obviously nor directly reinforced as such during training, during which time other very specific responses are, indeed, selectively reinforced.

The basic equivalence paradigm affords the subject feedback and reinforcement for very specific choices during training, but the *test* is *not* for those choices! Rather, tests for equivalence look for new choices, ones seemingly quite foreign to the training regimen. The tests for equivalence relations entail presentations of stimuli that were the options for conditional choice during reinforced training. In tests of equivalence, correct choices are novel; hence, they have never been reinforced during training.

In my view, the retrieval dimension for these novel responses is not to be found in changed values of some specific dimension (i.e., brightness) commonly used in studies of stimulus generalization, but, rather, apparently in a relation generated by operations of the subject's brain. Sidman accounts for this apparent dilemma by arguing that the traditional differentiations between stimuli and responses and even reinforcers do not apply to equivalence classes and relations, for they can include any or all of them as elements (p. 377). At this point, it is helpful to review and consider at least some of Sidman's other perspectives.

Sidman argues that "no additional experience on the part of the individual need be invoked in order to account for the observation that the components of a reinforcement contingency are related by equivalence" (p. 553). "The

problem . . . becomes . . . how to explain those particular instances in which some or all of the events involved in a reinforcement contingency *fail* to become members of the same equivalence class" (p. 554, italics added). The arguments of Hayes' (1992) relational frame theory and of James and Hakes' (1965) strategy of mediation are examined and then supplanted with an argument of his own.

Sidman does not believe that "stimulus control is best characterized as a linear process or structure" (p. 539). Neither does he argue that an element necessarily must belong to only one equivalence relation (p. 543).

> Particular discriminations . . . give rise to the abstraction of commonalities, often expressed in linguistic forms—words. From these words, we derive higher-order abstractions, a thought process that itself becomes available to us via equivalence relations. It is equivalence relations that make purely verbal constructions possible—words or phrases that have no immediate referents except for words or phrases. (p. 552)

Yet the role of language in equivalence relations is itself controversial (p. 362). Sidman views the successful work on stimulus relations reported by Schusterman and Kastak (1993) with a sea lion, in which it passed tests of identity, symmetry, and transitivity, and thus demonstrated equivalence, as very important to resolution of the issue. Schusterman and Kastak conclude that language is not a likely requisite for equivalence relations. Rather, they view equivalence relations as a requisite for language. They also hold that general intelligence is a requisite both for language and equivalence relations, a view shared by Savage-Rumbaugh (1986) and myself (Rumbaugh & Savage-Rumbaugh, 1994). Rumbaugh (in press) reports a high positive correlation between amount of additional brain afforded by encephalization and the probability that increased training will result in positive rather than negative transfer among primates in general.

Animal species (including humans) have their own unique evolutionary histories, a point acknowledged by Sidman. But on the next points, we might have some differences: I doubt that species are passive systems that simplistically service the contingencies of the environment without introducing their own biases of operation. Brains of different species likely have evolved to be differentially selective in their processing of sensory and perceptual input, and are likely well "designed" to "file, organize, and retrieve" lessons of the past in a manner so that they might generate new, creative, and adaptive responses to challenges of the present. (Sidman would not disagree with the last point.) Thus, it is reasonable to anticipate that brain function, notably in the primates, has, in large measure, evolved in ways that enable reinforcement and Sidman's elementary mathematical set theory to work in the manner that affords the bases for equivalence relations. But this is tantamount to asserting that equivalence relations emerge because of the structure and function of animal—and even human—brains. We know that encephalization, the disproportionate increase in brain size relative to body size (Jerison, 1985), appears to have enhanced the complex learning skills of animals and notably of primates (of which humans are one form). We also know that very early experience and prior learning play important roles in the formation of stimulus relations. Thus, it is the totality of the organism, nonhuman or human animal—not just reinforcement contingencies—that enables the emergence of stimulus relations.

Sidman's altered perspective is a major one. Because emphasis shifts from stimulus control to the emergence of relations between stimuli, many psychologists will view it as a break with traditional behaviorism, although Sidman appears not to view it that way. Indeed, he reports that his work on equivalence occurred before he related it to Skinner's formulations (p. 570). Nevertheless, the shift is reminiscent of the mid-century running debates between Hull and Tolman. For Hull, the

explication of learning entailed stimulus–response models and the formation of connections between stimuli and responses. By sharp contrast, Tolman argued for stimulus–stimulus models of learning, and for him there was more than one kind of learning. For Tolman, learning was perceptual and cognitive. It produced relations among stimuli (i.e., cognitive maps) and also between stimuli and responses (i.e., means-ends relations).

Just as Sidman and his associates at times found their enthusiasm and joy in research overwhelming, I must confess that I, too, was overcome with joy as I read of Sidman's altered perspective. Although Sidman eschews cognitive models, his work explicates phenomena that others view as cognition. (Tolman would smile, although Hull would scowl!)

Sidman lucidly conveys both his line of logic and the research that it engendered. His research defines the parameters within which human competencies can be developed and refined, even when mental retardation and sensory deficiencies might constrain the learner. A basic barrier to understanding what the learner has learned rests in our expectations about what subjects can and cannot accomplish. Too frequently it appears that what the subject learns is both equated to and apparently constrained by measurement systems that have a limited focus. But looking deeper, one can find exactly what Sidman did—the emergence of new conditional discriminations. To detect such outcomes, novel test conditions generally are required.

Reference to a serendipitous finding from our laboratory is offered to illustrate the point. Sidman's procedures typically entail the presentation of Stimulus A coupled with the subject's learning to choose Stimulus B, followed by the subject's learning to choose Stimulus C in association with the presentation of Stimulus B. The *serial* presentation of stimuli and choices is germane to Sidman's procedures. Emergent relations form. By contrast, David A. Washburn and I reported a project with rhesus monkeys presented *pairs* of numerals from the set 0 through 9 (Washburn & Rumbaugh, 1991). All choices except for 0 were reinforced. If the subject on a given trial saw 5 paired with 3 and chose 5, it would get five pellets. If it chose 3, it would get three pellets. (Pellets were delivered arrhythmically and were promptly eaten, in turn, by the monkey as they were delivered by the pellet dispenser.) From a reinforcement perspective, it was a win-win situation. All choices other than 0 were reinforced, although some received more reinforcers than others. The monkeys rapidly came to preferentially respond to the numeral of higher pellet value.

Seven possible pairings of numerals had been held back from training for the purposes of a final test to determine whether the monkeys had learned (a) which specific numeral from each specific pair of numerals, with which they had worked, netted the greater quantity of pellets, or whether perhaps (b) they could learn control by "relative values" that would allow them to optimize pellets received even with novel pairings of numerals. They could—and did! One monkey chose the higher numeral without exception, and the second made only two errors. To use Sidman's expression, stimulus relations were established experimentally—one between each numeral and the quantity of pellets to be delivered by its choice. Transitivity was inherent in the monkeys' successful test performances.

There was no pressure for them to learn to do so either by the operations of privation or by nonreinforcement if the numeral of lesser value was selected, other than what was inherent in the reinforcement values of different quantities of pellets that were associated with the various numbers, but they did so. After final tests with the novel pairings of numerals, both monkeys manifested a high sensitivity to the ranked values of numerals presented in random selections or groups of five at a time. Their first choice was for the

highest number, then the next highest, and so on until the set had been exhausted. For each selection, regardless of the order in which it was selected, the subject received an appropriate quantity of pellets. Had their sensitivity for such ranked preferences of numbers from sets of five not been tested for, we would have missed it. But because we view (a) the brain, and in particular the primate brain, as having ongoing functions that organize and interrelate experiences of the present with those of the past and (b) the products of those functions as selective determiners of what is learned, hence available for productive and adaptive use in novel situations, we looked for generalized competence in our monkeys, and found it. In sum, the sources of control of behavior are not always evident in the environment, and portions of them can be established by functions of the brain that have been selected for because of their value in survival and reproduction. What else the monkeys might have learned that we failed to test for is a moot point. Clearly the monkeys were sensitive to the overall relations between the pellet values of the numerals 0 through 9, and not just to the specific contingencies (i.e., quantity of pellets) associated with each choice.

To us it was surprising, but clear, that our subjects' brains were more than sufficient to the challenge afforded by our number program. They not only learned, but also learned relationally, and far more than the contingencies of the task required. Our rhesus monkeys learned the relations between quantities and numerals and, in turn, came forth with new dimensions of relations between stimuli—ones that provided for optimal choice in novel tests. It bears emphasis that the monkeys learned the complex relations between the numerals and their ordinally ranked values in a situation in which they could have done exceedingly well simply by choosing either number of each pair presented and by avoiding 0 when it was an option. How many pellets the monkeys got per trial also was seemingly of little consequence, at least from our perspective, in view of the fact that they were not deprived and trials were massed. More pellets were but a second away. (Subsequent research demonstrated that the relational learning occurred even without 0 as an option.)

At least two points of procedure contrast our study with monkeys and Sidman's study with children. One is that we did not present stimuli serially. The second is that all responses other than to 0 were reinforced, although with different numbers of pellets. Thus, the emergence of complex ordering relations, to use his term, appears not to be contingent upon the serial presentation of A, B, and C stimuli. Neither do they appear to be contingent upon the subject receiving no reinforcement for some choices.

Although our study is behavior, our subjects are very much alive and their brains are presumed to do much more than simply accommodate input-output systems of stimuli and responses. Our working assumption is that what they learn, and hence how they learn, is never simple. Frankly, I believe that my view that the brains of many animals, and primates in particular, have been selected via evolutionary pressures to organize (e.g., relate) sensory and perceptual input and to coordinate those, in turn, with response systems for successful adaptation appears to be far more compatible with Sidman's view than I had earlier thought. The only major point on which I suspect we disagree regards the specific role of reinforcement.

All of us agree that reinforcement is important and has a profound influence on what animals (including humans) do. Sidman views reinforcement as having two forms—*primary* and *conditioned.* Primary reinforcers are the "terminal events in operant and Pavlovian conditioning, whereas conditioned reinforcers acquire their reinforcing function" (p. 391). If Sidman is to attribute the complexities of

equivalence relations to reinforcement, its precise role and features gain more significance than is contained in the traditional definition that holds that a reinforcer is anything that increases the probability of a response. Both animals and people are known to learn highly complex things from observation alone, where responding is difficult to describe. Observing what another does cannot be equated with the pressing of a bar. And what is the reinforcement for such learning?

Is it necessary to posit that if there has been learning, there must have been reinforcement? If one can't find the reinforcer, as in observational (not imitative or copy cat) learning, is it defensible to insist that it must be something, somewhere? I don't think so. To do so can keep us from observing and understanding still other aspects of learning and behavior change.

The evidence is overwhelming that humans, and at least some nonhuman species, can learn from first-instance opportunities to observe. They can learn by watching others do things and by perceiving the flow of events across time, thereby establishing stimulus relations that are subject to differential recall and differential action. In short, if Sidman is correct in attributing the formation of equivalence relations to natural contingencies of reinforcement, then reinforcement will have to be defined as something more ubiquitous and subtle than are the events and items of reinforcement that are discretely dispensed in the operant laboratory.

The animal research literature is now replete with a rich array of studies that portray emergent relations between stimuli that, in turn, allow for the competent use of language and numbers in apes (Rumbaugh, in press; Rumbaugh & Savage-Rumbaugh, 1994, for a review; Savage-Rumbaugh, 1986; Savage-Rumbaugh & Lewin, 1994) and even in "insightful" problem solving by birds. All psychologists, even going back to Köhler, acknowledge the critical influence of context and experience upon the emergence of new competencies, whether they are termed insight, learning set, or something else. Sidman calls them equivalence relations and posits that they emerge within the contingencies of everyday life.

In science it is always proper to reexamine basic tenets. It is timely that psychology reexamine the definitions of stimulus, response, and even reinforcement, as Sidman has done in substantial measure in his book. How we go about our science declares its form and its validity. The salience of the stimulus, the salience of the response, and the functionally pragmatic influence of the reinforcer have served historically to capture the attention of behaviorally oriented psychologists. Although the history of our field has concentrated upon the three-term contingency (i.e., stimulus–response–reinforcement), our research subjects tell us that many kinds of learning, especially the more complex kinds entailed in the instantiation of cultural competence, are not limited to these terms.

The study of equivalence relations can encourage the emergence of new perspectives that are more symbiotic than competitive. In full acknowledgment of the important role and contributions made by those who identify themselves as experimental analysts of behavior, it is timely that rapprochements be worked toward, as indeed they are, to meld that perspective with others of our time. Sidman's book is a masterful account of his saga in research for understanding in the field of equivalence relations. To me, if not also to Sidman, there are implications for how our subjects' roles in experiments are to be viewed. Here it should be emphasized that the comparative-development framework can help us to understand the dimensions of brain evolution and development that enable the emergence of equivalence relations and relational frames of all sorts.

Both our research methods and our expectations about the nature of the learning process and the abilities of our

subjects can delimit what they might learn and what we, in turn, learn about their learning. If the behavior analyst is looking only for a change in a response, it is likely that only that will be measured. If the analyst is looking for emergent relations, novel tests will have to be designed and included in the research protocol. As these relations are discerned, it is good to remember that they are not responses that were correct or reinforced during training. Neither is the stimulus controlling behavior during training the one controlling behavior in the test if the prior training procedures enabled the formation of equivalence relations. In the manifestation of equivalence relations, there is neither stimulus nor symbol in the traditional sense. Rather, that to which the subject comes to respond because of equivalence relations is something new—a relation among stimuli, responses, or both. Just how the brain functions in the formation of equivalence relations, along with other types of learning, is presently unclear and promises to remain unclear sufficient to our early understanding of stimulus relations. This, however, does not keep us from defining the antecedents of such learning.

As Sidman's book shows, such a perspective directs our attention to questions regarding the subjects' abilities and the topics that capture their attention. Under optimal conditions, as noted by Sidman, even the constraints normally attending mental retardation did not keep subjects from learning much, much more "than anyone had previously succeeded in teaching them" (p. 43). Complex learning can be exclusively about the relations between stimuli and environmental events. In my view, these relations are not necessarily dependent upon the operations of reinforcers, at least not in the same manner that they appear to affect the shaping and occasion for motor learning and performance. And it is reasonable to anticipate that the same procedures that result in nothing more than the establishment of stimulus control over a response in one kind of subject population (i.e., per species and developmental levels) might engender complex, hierarchically organized stimulus equivalence relations in others. Operations do not necessarily entail the same consequences (Rumbaugh & Pate, 1984) for all subjects or for all species.

Conditioned responses, in a sense, also emerge from experiences, as do stimulus relations. For stimulus relations, however, brain complexity (Rumbaugh, in press) is relatively more important, a point with which Sidman would likely agree (p. 567). Responses are conditioned, but capacities for complex learning are the products of evolution and are subject to being cultivated or structured in various directions, generally by protracted histories of experiences or rearing. Competence for complex learning is not established by conditioning.

At the outset of procedures in which responses are to be conditioned, at least elemental parts of the target response might be seen. By contrast, the potential or the capacities that are requisite for the emergence of equivalence relations, though posited, can never be seen. Notwithstanding, it is fruitful to posit their existence.

Eventually, bright subjects, be they human or nonhuman, appear to learn everything about everything, and that learning probably entails complex relations among stimuli and among responses and is unlikely to be constrained to single, specific responses. In the flatworm, although the conditioning of responses is possible, the emergence of equivalence relations would be most unlikely. And although one can condition the loci to which a pigeon will home, one does not establish homing by reinforcing it as though it were a motor response. Homing is not a response, but rather is a genetically dictated capacity or potential that characterizes some species, although not in identical ways.

Most of us are satisfied to become journey-persons in our research and

scholarship. A few among us become masters. Sidman is masterful throughout his book, where his mission is to communicate the joys, sorrows, frustrations, and excitement of research, while also opening up to other behavior analysts and teachers the profound merits of new paradigms for teaching new competencies in those students whose learning is compromised by mental retardation. Although the structures of his experiments and arguments are complex, they are conveyed clearly. He faces questions and counterarguments squarely. His answers have the force of conviction and are compelling. His work provides a sterling model to be emulated by others.

The text will be of great value for instruction at the upper-division and graduate levels. Its impact will be substantial, for it defines an important advance in our efforts to understand the richness of behavior in both humans and nonhuman animals. Although not presented to that end, the book might also serve to bridge communications with other groups of animal researchers whose interests lie more in a comparative or ethological framework. Sidman's research and perspective are timely, yet overdue. Were it the case that the genius of his book had been available to psychology 20 years ago! That said, his book is now before us—neatly, honestly, bravely, and wisely presented. All behavior analysts, present and future, should study it in detail, because it presents both a model and a road map for a successful future for the experimental analysis of behavior, a future in which no one need fear, "What would happen if astronomers tried to decree it unlawful to talk about sunsets?" (For the behavioral *meaning* of this question posed by Sidman, read his book and, in particular, p. 571.)

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